Dear referee #1,

Thank you for the constructive comments that will help us to improve the manuscript. Please find our responses below. We will revise the manuscript accordingly after the review process is closed by the editor; meanwhile we would be happy to get your response for the few cases where we disagree.

Referee #1:

General comments

I found this paper well written and containing useful results. Design of the study is optimal for chosen object and helps to represent studied phenomena. Methods were described good enough. Especially I appreciate honest remarks on several methodological details. Literature was presented comprehensively, all important available studies were cited and used for comparison. It is very nice that photo of studied object was given in the paper (but in comments below I strongly recommend to give more photos to give a reader as much as possible information on your object). Nevertheless I recommend to improve this manuscript in several directions (they are described in specific comments section). After that I think this paper should be published.

Specific comments

Comment: Lines 54-55. It seems to me that in these lines it was pointed out that during methane transport in vascular plants methane is not consumed by methanotrophs. I think this is wrong because there are a lot of papers showing presence of methanotrophs in plant tissues (for example, Bao et al., 2014; Doronina et al., 2004; King, 1994). Am I right?

Response: Yes, this is true for several wetland species such as rice *Oryza sativa* (Bosse and Frenzel, 1997) and cattail *Typha latifolia* and *Calamagrostis canadensis* (King, 1994). However, in bogs so far significant methane oxidation has not been detected in vascular plants, such as *Eriophorum angustifolium* and *E. vaginatum* (Frenzel and Rudolph, 1998), but in *Sphagnum* mosses (Larmola et al. 2010). We will included this in the Introduction.

Comment: Lines 110-115. I think your object should be described in paper text more comprehensively. I understand that there are a lot of papers about Siikaneva station peatland. But more information important for YOUR study should be given. In general you describe your research sites as peatland. But you also compare methane fluxes from your site with fluxes from ponds and lakes. It seems to me that there is an ambiguity. Do you think that your objects are something between shallow lake and peatland?

It is very important for future reader to understand what EXATLY is your object. That is why more information on factors of methane emission should be given in the paper text. What was the water depth in all three site types (OW, EW and BP)? Why bare peat is bare? It is not typical for natural intact wetlands in Canada or Russia, where water table depth is about 0 cm or higher and moss cover is continuous. Why these parts of peatland are so wet (or so submerged)? Does peat on your sites removed by erosion (or by any secondary process, but not in inherent peatland development)? If so it decreases methane emission (because relatively young and rich in substrates peat layer was removed). Or your sites are in an inner water channel (stream) inside the peatland?

I strongly recommend to add photos of bare peat surface and water's edge (EW in your terms) sites and probably small map of your peatland to see where your sites are situated.

Response: Our study site is a patterned peatland with parches without moss cover. Those patches are either shallow with visible peat surface having their water level at the surface or deeper, covered by open water. In a way, our objects are something between very small and shallow lake and peatland.

Water depth in BP in 2014 was on average -1.8 cm. Water depth in the pools is not easy to measure as it is hard to determine what is the bottom of the pools: on average there is one meter of water over very loose peat slurry. The water depth does not generally differ between EW and OW as these peatland pools do not have shallower banks, but are deep right away at the edge of the surrounding moss cover. The shallow arctic lakes in Wik et al. (2013) have organic sediment layers and their water depth varies around 0-6 m making them not that different from our pools except being much larger in their area.

The bare peat surfaces or 'mud-bottom hollows' found in our site are commonly described in boreal peatlands in, Estonia, Finland, Sweden, Russia, Western Europe and North America (Karofeld et al., 2015 and the references therein). Their formation processes are not well known but according to Karofeld et al. (2015) at least in this region they are not formed due to erosion but are inherent part of peatland development.

We will include a more detailed description of our study objectives and the aerial photo of the site (Figure 1. below) to Methods.

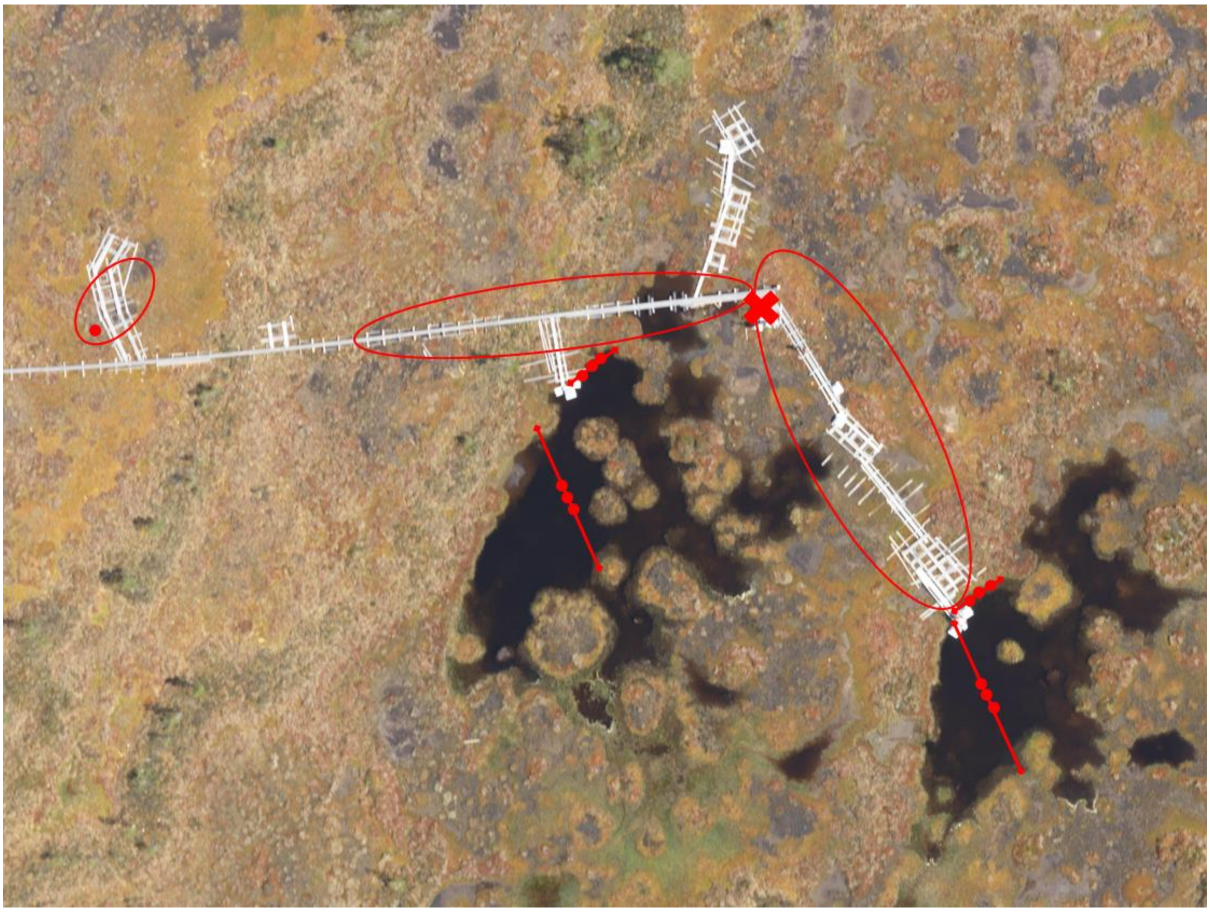

Figure 1. Aerial photo of the study site in Siikaneva bog. Red lines with dots mark the floating gas traps in open water (OW) and water's edge (EW). Red circles mark the area within what the gas traps were placed on bare peat surfaces (BP) that are seen as brownish-grey in the photo. The eddy covariance (EC) raft is marked with the red x.

Comment: Lines 127-128. How do you define where is open water (OW) and where is water's edge (EW)? Based on water depth? It is important because methane emission is known to be WT-dependent (see Wik's papers from your reference list for example).

Response: EW and OW were not defined based on the water depth but simply by the distance to the nearest vegetated surface. EW was right next to the moss and OW was the furthest away from it in the middle of the pool (at least 1m, on average about 3 m away from the edges). This separation was chosen to reflect the potential difference in availability of substrate for methanogenesis.

Comment: Lines 137-139. I have several questions to discuss on using triggered ebullition gas concentration for calculation of the total emission.

a) As I understand bubbles release to the surface from the peat when methane concentration in them reaches a certain threshold. If it is right, triggered ebullition gas concentration is lower than "real" ebullition gas concentration because gas concentration in triggered bubbles did not reach a certain threshold. Hence methane and CO2 concentrations and their fluxes are underestimated using your methodology (not by much I think). If I am right please mention it in the paper text.

b) Methane is poorly dissolved in water. And concentration of dissolved methane in peatland water is usually high and close to saturation level. That's why I think that methane concentration in funnels during a week (actually it is less than a week, because bubbles do not release right after funnel installation) can be more or less constant and not decreased by diffusion. Did your compare methane concentration in funnels after week of exposition and methane concentration after triggered ebullition in the end of this week? I have never read about methodology you used and think that it is novel. Any novel methodology should be assessed. For example, Martin Wik (see papers cited in your manuscript) use methane concentration in funnels after week (or couple of weeks) of exposition. It is always risky to use not in situ concentration in such heterogeneous environments as peatlands.

Response:

a) Bubbles are not released from the peat after reaching certain methane concentration but depending on the bubble volume (Peltola et al., 2018). Volume in turn is controlled by pressure and temperature that affect methane solubility.

b) We have measured methane concentration of the gas caught in the funnels in 2014 and compared those with the fresh samples triggered from the peat. The concentrations in the funnels were clearly lower than in the fresh ebullition samples (see the table below). Therefore, after 2014 we did not measure concentrations from the funnels anymore but only took fresh bubble samples to get more accurate concentrations. We will include this in the methods; Table could be added as an appendix.

	Mean CH4 concentration ml/l			
Date	**W funnel**	**BP funnel**	**W fresh**	**BP fresh**
10-Jun	17	45	293	268
16-Jul	61	*245*	420	313
13-Aug	86	152	399	172
05-Sep	49	*63*	379	221

Numbers in Italic indicate the concentration in a single measured funnel, not a mean of many funnels

Comment: Line 158. Where exactly your water table sensor was placed, in what site? As I see on Figure 3 you use the same WT data for prediction of fluxes from all three types of sites. But I think it is not correct because each site type has own WT mean level and WT seasonal dynamic. Anyway it must be mentioned in a paper text.

Response: Water table sensors were installed in a lawn about 1.5 m away from the EC raft. Unfortunately, it was impossible for us to measure the distance of WT to peat below as the bottom was formed by loose peat slurry. Therefore, we considered that the continuous WT logger was the most reliable source of data on the seasonal variation in WT.

References

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